PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Incidence of newborn resuscitative interventions at birth and short-term outcomes – A regional population-based study
AUTHORS	Bjorland, Peder Aleksander; Øymar, Knut Asbjørn Alexander; Ersdal, Hege; Rettedal, Siren Irene

VERSION 1 – REVIEW

REVIEWER	Reviewer name: Elizabeth Foglia Institution and Country: Children's Hospital of Philadelphia, USA Competing interests: None declared
REVIEW RETURNED	01-Nov-2019

GENERAL COMMENTS	Bjorland and colleagues present an observational study describing the epidemiology of resuscitative interventions performed in an unselected population of infants born in a single hospital in Norway over a 1-year period. The topic is significant, as surprisingly little is known about the incidence and site variability of resuscitative interventions performed among late preterm and term infants. The paper is well written and the data are clearly presented. It is a bit surprising that infants were returned to their parents directly even after receiving cardiac compressions, but perhaps this highlights the need for better data to inform the decision of which infants need extended monitoring after resuscitation. I have only 2 minor comments for the authors to consider: 1. Table 3: it would be helpful to clarify if these were cord blood samples or samples taken directly from the infant (ie: at some point during/after resuscitation). 2. The authors may expand on the limitations of missed video recordings. Were the video recordings preferentially captured in specific rooms or settings? In our setting, video recordings are less likely to be recorded if there is an emergent delivery, due to limited time to set up and initiate the recording. Is it possible such a bias exists in their study?

REVIEWER	Reviewer name: Anne Lee Solevåg Institution and Country: Rikshospitalet, Norway Competing interests: No competing interests
REVIEW RETURNED	06-Nov-2019

GENERAL COMMENTS	Dear Editor of BMJ Paediatrics Open and the authors of the original paper entitled 'Incidence of newborn resuscitative interventions at birth and short-term outcomes – A population-based study' by Bjorland and coworkers. Thank you for the opportunity to review this prospective observational study aiming to determine the incidence and characteristics of resuscitative interventions at different gestational ages and short-term outcomes after resuscitation.

	The study utilized incidence report forms for every delivery, as well as video recordings of a convenience sample of neonatal resuscitations during a 12 month period in a tertiary Norwegian hospital. Short term outcomes included the fraction of newborns from 34 weeks´ gestational age that were returned to parental care without further follow up. One of the main finding of the study was that late preterm and post term deliveries required more intervention. However, most infants were not severely affected and short term outcomes were good. The authors conclude that "The high incidence of resuscitative interventions in premature newborns ≤34 gestational weeks supports centralizing those deliveries to centres with adequate expertise and high level of preparedness to provide optimal stabilization and resuscitation at birth". Considering the fact that most infants required minor/basic interventions, I wonder whether this conclusion can be supported by the results. The most severely compromised infants (n=8 needing full CPR) were (near) term. While acknowledging the great amount of work associated with such a study, I would have wanted more information about the methods used for e.g. data analysis. Was video analysis standardized? Did the authors keep any form of transcripts of the video recordings? Also, I wonder if the rate of consent after discharge would improve if the parents were contacted by phone, not exclusively by letter. Was calling the parents considered intrusive/unethical? Below are some (minor) comments to the different sections of the manuscript: INTRODUCTION Page 3 Line 26: "Current estimates are widely based on studies conducted in low- or middle-resource settings, though recent studies imply that this is a frequent concern also in high-resource settings (13-16)" Comment: What is also a frequent concern also in high-resource settings? MATERIALS AND METHODS Page 3 Line 57: "Stavanger University Hospital serves a population of 350.000 with approximately 4600 deliveries annually, and is the only hospital in the region with delivery and newborn services" Comment: What region is the authors referring to? If Stavanger University Hospital is a level 3 hospital, there should be other delivery and neonatal wards in the region that make referrals (i.e. extreme preterm births) to Stavanger. Page 4 Line 26: Fisher&Paykel not Fiscer&Paykel Page 5 last paragraph: "During video analysis, the newborn`s respiratory effort at placement at the resuscitation bay was characterized as adequate, inadequate (e.g. grunting or severe retractions) or apnoeic. Drying and stimulation was considered adequate if the newborn received tactile truncal stimulation (drying, chest- and back rubs) prior to respiratory support" Comment: Did PAB use a standardized score sheet? Were some of the recordings analyzed by (at least) two independent researcher to ensure reliability?

	Analysis (page 6): I suggest that the authors include what statistical tests they used. This information can be found in the figure legends, but should perhaps also be mentioned in the methods section. Page 6 line 35: its' legal and ethical concerns have been debated Comment: Should be 'its legal and ethical concerns have been debated' RESULTS Page 7 Line 48: Respiratory support alone, either by CPAP or PPV was sufficient in 95% of the resuscitated newborns, whereas the remaining 5% received (additional) circulatory support by chest compressions (n=10) or intravenous fluid and/or adrenaline boluses (n=15). Comment: I suggest deleting the word "additional" Page 8 Line 30: "Upon arrival on the resuscitation bay, 161/192 (82%) newborns were adequately dried" Comment: Why is the denominator 192, and not 196? Typo? Page 8 Line 50: "Median intubation time for each attempt was (of) 40 seconds (33-55)" Comment: I suggest deleting the word "of" Page 9 Line 3: "Eight of the newborns who received full cardiopulmonary resuscitation were video-recorded" Comment: How many newborns received full CPR in total? Please also define "full cardiopulmonary resuscitation". Page 9 Line 43: "Pneumothorax was diagnosed in three of the resuscitated newborns." Comment: Did pneumothorax occur in infants that received PPV in the delivery room? Complication to resuscitative interventions? DISCUSSION Page 10 Line 52: "In our study, more than 30% of the newborns receiving CPAP were evaluated as adequate breathers by the viewer, highlighting the uncertainty of the necessity." Comment: How was "adequate breathers" defined? Maybe they needed supplemental oxygen or were having mild retractions that could not be seen in the video recordings? I would suggest revising the term "uncertainty of the necessity" Page 11 Line 12: 'different patient`s characteristics, is unknown' Comment: Should be 'different patient characteristics, is unknown'. Niles et.al. should be Niles et al. Page 11 Line 24: "Our findings are similar to previous findings from Norway by Skaare et.al. for both incidence and duration of PPV (14, 23)"

	Comment: I think perhaps the authors could be more clear about the rationale for performing a very similar study in a similar context as the one reported by Skaare et al Page 11 Line 40: 'the recommended time of PPV before initiating chest compressions were prolonged' Comment: Should be 'the recommended time of PPV before initiating chest compressions was prolonged' CONCLUSION (not CONCLUTION) Page 13 Line 6: "The high incidence of resuscitative interventions in premature newborns ≤34 gestational weeks supports centralizing those deliveries to centres with adequate expertise and high level of preparedness to provide optimal stabilization and resuscitation at birth." Comment: Do the authors suggest that all deliveries ≤34 gestational weeks should be centralized? What is 'adequate expertise'? Table 4: Are the base excess values supposed to be negative (base deficit)? In conclusion, a well-written paper, and probably a well conducted study. However, more information about the data analysis should be provided to judge the methodological quality. I think that the study conclusions could be improved. Based on the rich data obtained with the incidence report forms and video recording, I expect the authors to make suggestions for practice that are even more interesting and relevant. E.g., I find the higher need for intervention in post term infants interesting. Do the authors think that this finding should inform obstetrical practices (when to induce post term pregnancies)? What do other studies tell us about post term pregnancies and neonatal compromise?

REVIEWER	Reviewer name: Paul Cawley Institution and Country: University Hospitals Bristol, UK Competing interests: None
REVIEW RETURNED	12-Nov-2019

GENERAL COMMENTS	Many thanks for your hard-work in defining the incidence of newborn resuscitation in your regional population. I have made suggestions for both major and minor revisions to your manuscript, which I found informative. General comments which support consideration of this study's publication after revision: Incidence of newborn resuscitation in high, middle and low resource settings has been previously reported. The major potential advantage of this study over other published works is the combination of prospective data collection with video analysis. Whilst the study is based within a small regional population, it has a relatively large series of video analysed resuscitations which enhances the validity of reported incidences of delivery room interventions and their characterisation. Other similarly large published series of video analysed resuscitations have reported only on one aspect of resuscitation (such as infant position or suctioning).

	Overall, this paper has the potential to overcome some of the limitations which are present in our current evidence base: - Studies where data collection of resuscitation events is based on retrospective review of medical notes (subject to underreporting & inaccurate notation) - Studies based on solely high-risk sub-populations; reducing generalisability - Video-analysis studies which have included only small study samples, have focused on healthcare professional feedback & learning, are limited to a particular sub-population, or focus on only one or two interventions - Studies in which outcomes have not been declared In its present form, however, the manuscript has not fully utilised the opportunities presented by video-recording delivery-room resuscitation events. Whilst incidence is reported, it does not fully characterise resuscitation interventions. The following major and minor revisions are suggested to improve the manuscript further. Suggested improvements for Authors: Major Revisions: 1) The first objective of this study is I) to describe the incidence and characteristics of newborn resuscitative interventions during the first minutes of life. Incidence of CPAP, PPV, Chest compression, IV adrenaline and IV fluid bolus are described. However, further characterisation of the resuscitation events is needed within the results section. Depending on the availability of concurrent physiological parameters, and definition of video recordings, the following should be considered for inclusion in the study report where possible: Timing of intervention from birth, response of infant & quality of delivered intervention: Primary resuscitative interventions: -Time from birth to arrival at resuscitation bay -Time from birth to initiation of PPV -Time from when PPV started to when chest rise first seen or heart rate increase seen? -Time from birth to initiation of chest compressions -In infants who received chest compressions – how long had effective PPV been administered prior to starting chest compression? -Time from birth to first attempt at IV/IO access -Time from birth to first successful IV/IO access (and what type?) -Time from birth to first adrenaline dose / Fluid bolus -In apnoeic infants with bradycardia or without cardiac output; time to first detectable heart rate, time to HR>100 bpm, time to gasping, time to spontaneous breathing? Secondary resuscitative interventions (of lesser importance but will still be of interest to many readers): -Time from birth pulse-oximetry attached & time that reliable pulse-ox signal established -Time from birth ECG attached (if attached)

	-Frequency/time interval of Heart rate checks by stethoscope if no reliable ECG/Pulse-oximeter -Time from birth CPAP started (in CPAP only group) -Duration of CPAP delivered in CPAP only group -Initial grade of breathing (inadequate/adequate) + response to CPAP in CPAP only group (did they become adequate breathers?) -How many infants were started on CPAP without PPV but then subsequently became apnoeic? -Incidence of airway maneourves needed to achieve chest rise: Jaw thrust/oropharyngeal airway/ two-person facemask hold & PPV technique -Starting FiO2 (air or more?) -Incidence of suction: use of laryngoscope during suction? -Incidence of response to suction (Distress in breathing improved? PPV subsequently successful or bradycardia occurred?) -Quality of chest compressions delivered (Rate, depth, recoil) -Ratio of compressions to breaths used -Dose of first and subsequent adrenaline boluses -Number of adrenaline boluses The above data could be presented almost entirely in tabular form or graphically as a box and whisker plot. Currently there are no published prospectively collected/video scrutinised data on timing and quality of full resuscitation interventions in the delivery room. By providing more in-depth characterisation of resuscitation interventions, the authors' would fulfil the potential offered by video-recording the resuscitations, and fulfil their primary objective. In addition, the authors' discussion points would be strengthened, for example: -Page 10, line 52-56: "In our study, more than 30% of the newborns receiving CPAP were evaluated as adequate breathers by the viewer, highlighting the uncertainty of the necessity." – These data are not presented in the results section, only here in the discussion for the first time. In addition, no distinction is made between CPAP use in term and preterm infants; The Norwegian Resuscitation guideline advises that Initial respiratory support of spontaneously breathing preterm infants with respiratory distress may be provided by CPAP. -Page 11, Line 35-39: "Still, three of the newborns in our study received chest compressions for less than one minute, suggesting proper airway handling might have been sufficient." - Can this be concluded if there is no comment on how the airway and ventilation of the infants were managed? In these 3 infants with <1 min chest compressions, how long had effective PPV been administered prior to initiating chest compressions? This data should be provided in the results section. -Page 11, Line 39-47: "When ILCOR revised their guidelines on newborn resuscitation in 2015, the recommended time of PPV before initiating chest compressions were prolonged, recognizing that most compromised newborns will respond to adequate ventilatory support alone (4). Our findings may support this recommendation." - The findings of this study can only lend support to this recommendation if data are provided on duration of effective PPV prior to initiation of chest compressions, and the effect on heart rate. I note the 2015 revision of the NRR recommends:

	‘Start chest compressions only after you are sure that you have ventilated the child adequately for 90 - 120 seconds’. Were there infants in your population who were bradycardic up until 90-120 seconds of PPV but then were no longer bradycardic and thus did not require chest compressions? -Page 8, lines 52-54: “failed mask ventilation n=4, chest compressions (n=3) or need for prolonged respiratory support (n=3) ” - Any indication of why face mask ventilation had failed? Technique, anatomy? How long is prolonged respiratory support? 2) Short-term outcomes should be defined within the manuscript’s method section. The second objective of this study is II) to assess short-term outcomes after resuscitation. Short-term outcomes are not defined within the manuscript’s methods, but are reported as ‘immediately returned to parents’ Vs ‘NICU admission’ for infants ≥34 weeks gestation in the results section, alongside NICU length of stay, need for mechanical ventilation, occurrence of pneumothorax, provision of therapeutic hypothermia, Magnetic Resonance brain scan reports & death. Within the discussion, short-term outcomes are described as ‘Survival with rapid recovery, need for intensive treatment or death’. Short-term outcomes are not differentiated for infants <34 weeks’ gestational age. Consistent definitions of short-term outcomes should be used and declared for all gestations. (e.g. Survival – returned to parents, survival – Neonatal Unit admission (& characterise level of intensive care), Length of stay, and Death in first 28 days) 3) A stronger acknowledgement of the study’s limitations is required within the discussion. In particular: -Video-analysis was achieved in 196 of a total of 295 resuscitations: 33.6% loss of video-analysis overall. -20 intubations took place in the study period, only 10 of these were video-analysed: 50% loss to video-analysis. -10 infants received full cardiopulmonary resuscitation, 8 of these were video-analysed: 20% loss to video-analysis -Page 12, line 38-40. "Video cameras were not available at all resuscitation bays, but this should not cause a bias. There was a poor response rate when consent was asked by letters. This typically included newborns who were discharged early from hospital, and may represent a group where lesser interventions were required. Importantly, the results regarding incidences of resuscitative interventions were not consent- or video-dependent and therefore not affected by these potential biases" – This requires further qualification. If loss of video analysis is not a cause for potential bias or inaccuracy, why use video-recording at all? Loss of video analysis occurred in 50% of infants who were intubated and 20% of infants who received full resuscitation – these are infants who do not ‘represent a group where lesser interventions were required’.

	In infants who had video-analysis, did the recorded interventions in the 'observational incidence report form' correlate with the video analysis? (This would provide a measure of risk of bias in the 33% of resuscitations within this population for which video analysis was missed) 4) The discussion should further exploration how generalisable the findings of this study are to other populations. The authors may like to comment/compare how the incidence of fetal distress, as characterised by umbilical cord gases & 1-minute Apgars, in this population compares with international data. From the data presented in the manuscript, evidence of fetal distress appears low – even in the eight infants who received full cardiopulmonary resuscitation, only one infant demonstrated severe cord blood acidaemia (pH<7.00), and three (38%) had arterial cord pH ≥7.2. The caesarean section rate was 13.7%, with 9.6% acute caesarean sections – the authors should reflect in their discussion on how their population compares with other high resource settings and if they feel this may affect the generalisability of their findings. Minor Revisions: Grammar/spelling/writing style: Page 3, Line 55 "Stavanger University Hospital serves a population of 350.000" – suggest 350,000 Page 3, Line 35-37 "Further, short-term outcomes of near- or at term newborns after resuscitation are poorly described (17, 18)" – Consider revising the word 'Poorly' - use of the adjective 'poorly' implies criticism of prior published works - is this the intention or is the intention to highlight a gap in the current literature? Page 4, Line 6 "Newborns with antenatal diagnosed severe" – suggested 'antenatally diagnosed' Page 4, Line 19 "and a backup resuscitation bay in case of for example twin deliveries" – suggest "and a backup resuscitation bay in case of, for example, twin deliveries" Page 6, Line 18 "Results are presented as number (%), median (IQR) and mean (95% CI)." – suggest defining the abbreviations IQR & CI at first use. Page 7, Line 43: table description "*Umbilical blood sampels were not available for all patients" – samples Page 10, line 24 - table description, Fisher's exact test not fischers. Page 10, Line 47-49 "We defined CPAP as a resuscitation intervention, as it is suggested in the ILCOR guidelines as a mean to" – suggest ' as a means to' Page 10, Line 55-57 "highlighting the uncertainty of the necessity. " – suggest 'highlighting the uncertain necessity of the intervention' Page 12, Line 52 "CONCLUTION" – Conclusion

	Page 13, Line 26 "AUTOR CONTRIBUTION" – Author Contribution Methodological descriptions: Please specify if any high-risk deliveries were transferred in from other regions. The authors should define how CPAP was delivered – was it humidified, nasal or face mask or simply by applying the NeoPuff T-piece resuscitator with adjustable PEEP valve? “Full cardiopulmonary resuscitation” is used on several occasions without full definition; please define on first use or within the methods section. Page 6, Line 42 "For patient and personnel safety considerations" - what safety considerations mean video recordings need to be erased after 3 weeks? Should this be for data-regulation /privacy purposes? “Resuscitation Bay”: Bay usually defines a physical space or alcove. A “Resuscitation bay” would normally describe the physical space in which the equipment is stored/used, as opposed to the equipment itself. Resuscitation cot/crib/unit may be more appropriate terms? Scientific Corrections/clarifications: Page 7, Line 3: "During the study period, 4610 deliveries took place in our region, resulting in 4697 live born" – Should this be ‘4710 deliveries took place’? Page 7, Lines 4-5 "12 were transport deliveries" – what are transport deliveries? Page 7, Lines 17-19, "In total, 295 (6.3%) newborns received resuscitative interventions. The characteristics of the resuscitated newborns are shown in Table 1." Readers may be confused that only 256 participants are included in table 1, when earlier in the sentence 295 are referenced. Consider clarifying that table 1 includes only the infants for which consent was provided to access further details? Base Excess: In the manuscript, many of the reported Base Excesses are positives, even in infants with acidotic pH. Should these Base Excesses be negative? Presumably the acidotic infants did not have an excess of base (i.e. an alkalosis) and so had a negative base excess? Amendments to tables: Table 1 Consider presenting the normal range (2 Standard deviations) of umbilical cord gas data (or alternatively the IQR and minimum and maximum values) as the IQR doesn’t provide information on the 50% of the infants who had the most extreme or most normal gases. Table 2 PPV on 28-33 gestation group: n=26.Percentage also stated as 26% - should be 40.6%?

	Table 3 Why have a "-" to indicate no adrenaline bolus for four infants and a "0" for one infant?. Consider including Apgar scores. Table 4 Page 10 Table 4: "Table 4 - Comparison of newborns above 34 weeks gestation returned to parental care or admitted to newborn intensive care unit (NICU)" - Should be equal or above 34 weeks / newborn or neonatal (as described on page 4)? Statistical analysis: Why was Chi-Square used for some data points and Fisher's Exact used in others? References: Page 6, Line 35: Reference 22 does not appear related to the ethics of video recording Page 12, Lines 13-24: Reference 29 is not primarily related to functional reisdual capacity or primary apnoea after birth Title: Consider defining the population within the study title (readers may anticipate a higher participant number from the current title) – For example: 'Incidence of newborn resuscitative interventions at birth and short-term outcomes – A regional population-based study' Consider as electronic supplementary files: Photo of the resuscitation bay & equipment Copy of Norwegian Resuscitation Council Guideline Copy of observational incidence report form Funding: "PAB received a PhD grant from Laerdal Foundations" - Please clarify whether Laerdal had any involvement in study design, analysis or conclusions?

REVIEWER	Reviewer name: Peter Flom Institution and Country: Peter Flom Consulting Competing interests: None
REVIEW RETURNED	13-Nov-2019

GENERAL COMMENTS	I confine my remarks to statistical aspects of this paper. These were very simple, but appropriately so. I recommend publication.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Bjorland and colleagues present an observational study describing the epidemiology of resuscitative interventions performed in an unselected population of infants born in a single hospital in Norway over a 1-year period.

The topic is significant, as surprisingly little is known about the incidence and site variability of resuscitative interventions performed among late preterm and term infants.

The paper is well written and the data are clearly presented.

It is a bit surprising that infants were returned to their parents directly even after receiving cardiac compressions, but perhaps this highlights the need for better data to inform the decision of which infants need extended monitoring after resuscitation.

I have only 2 minor comments for the authors to consider:

1. Table 3: it would be helpful to clarify if these were cord blood samples or samples taken directly from the infant (ie: at some point during/after resuscitation).

Reply: The blood gas values listed in table 3 were cord blood samples taken immediately after birth. This has been clarified in the method section.

2. The authors may expand on the limitations of missed video recordings. Were the video recordings preferentially captured in specific rooms or settings? In our setting, video recordings are less likely to be recorded if there is an emergent delivery, due to limited time to set up and initiate the recording. Is it possible such a bias exists in their study?

Reply: Our cameras were mounted on the resuscitation cribs during the whole study, and recordings were triggered by motion on the resuscitation crib. This way enabled us to record both “expected” and “unexpected” resuscitations in contrast to studies mainly focused on high risk deliveries. Due to technical and financial limitations however, not some “reserve” resuscitation cribs were not equipped with a camera. Consequently, some newborns treated at reserve resuscitation bribs (n=12) were not recorded (i.e. if main resuscitation crib was currently occupied, twin deliveries, Caesarean sections in additional operation theatres etc.). However, this was at random, and should in our opinion not create any bias. In addition, some of the NICU`s mobile resuscitations cribs used for premature newborns <34 weeks of gestation were not equipped with cameras. This resulted in a relatively higher missed video recording rate for premature newborns (n=23) than for near-term and term newborns, and could potentially lead to an underestimation of the characteristics of the resuscitative interventions. We have now rightfully added this in the limitation section. Finally, some episodes were not recorded due to technical failure of the equipment (n=13), also at random, and therefore in our opinion not a source of bias. Importantly, the main outcome in our study; the reported incidence of resuscitative interventions, were not depended on video recordings.

Reviewer: 2

Dear Editor of BMJ Paediatrics Open and the authors of the original paper entitled ‘Incidence of newborn resuscitative interventions at birth and short-term outcomes – A population-based study’ by Bjorland and coworkers.

Thank you for the opportunity to review this prospective observational study aiming to determine the incidence and characteristics of resuscitative interventions at different gestational ages and short-term outcomes after resuscitation.

The study utilized incidence report forms for every delivery, as well as video recordings of a convenience sample of neonatal resuscitations during a 12 month period in a tertiary Norwegian hospital. Short term outcomes included the fraction of newborns from 34 weeks´ gestational age that were returned to parental care without further follow up. One of the main finding of the study was that late preterm and post term deliveries required more intervention. However, most infants were not severely affected and short term outcomes were good.

The authors conclude that “The high incidence of resuscitative interventions in premature newborns ≤34 gestational weeks supports centralizing those deliveries to centres with adequate expertise and high level of preparedness to provide optimal stabilization and resuscitation at birth”. Considering the fact that most infants required minor/basic interventions, I wonder whether this conclusion can be supported by the results. The most severely compromised infants (n=8 needing full CPR) were (near) term.

Reply: As the reviewer correctly points out, most infants required basic intervention and the most severely compromised infants were near term. However, newborns <34 weeks of gestation shows a higher chance of PPV and intubation, demanding experience and skills exceeding basic resuscitation skills. Additionally, we know that initial stabilization of extreme premature newborns has great impact on mortality and morbidity. However, we acknowledge that the findings in our study are insufficient to draw such conclusions by itself, and we have removed this sentence in the revised manuscript.

While acknowledging the great amount of work associated with such a study, I would have wanted more information about the methods used for e.g. data analysis. Was video analysis standardized? Did the authors keep any form of transcripts of the video recordings?

Reply: For this study, we issued a Standard Operation Protocol (SOP) standardizing recorded variables during video analysis. We registered all extracted data in a computer-based registration sheet, designed for this study. We did not find it appropriate to attach the SOP to supplementary files, as it is written in Norwegian, and contains data exceeding the content of this paper. However, relevant content is now included in the method section.

Also, I wonder if the rate of consent after discharge would improve if the parents were contacted by phone, not exclusively by letter. Was calling the parents considered intrusive/unethical?

Reply: We aimed to approach all parents prior to discharge from hospital. However this was not always practically achievable, and consent would then be asked by letter. In council with the hospital data protection officer we were advised against contacting parents by phone, as this was considered intrusive. Instead, a second letter was sent in a final attempt to obtain consent. Unfortunately, we either failed to obtain consent (n=35/295), or the consent were returned by letter after the 3-weeks period prior to video record deletion (n=11/295). The latter 11 episodes could have been included if we could have extended the time limit for storing video recordings allowed by the hospital data protection officer (3 weeks). We will certainly argue for an extended storage period in future studies.

Below are some (minor) comments to the different sections of the manuscript:

INTRODUCTION

Page 3 Line 26: “Current estimates are widely based on studies conducted in low- or middle-resource settings, though recent studies imply that this is a frequent concern also in high-resource settings (13-16)”

Comment: What is also a frequent concern also in high-resource settings?

Reply: Recent studies imply that newborn resuscitation is a frequent concern even in high-resource settings, with a higher level of foetal monitoring. We have clarified this in the manuscript.

MATERIALS AND METHODS

Page 3 Line 57: “Stavanger University Hospital serves a population of 350.000 with approximately 4600 deliveries annually, and is the only hospital in the region with delivery and newborn services”

Comment: What region is the authors referring to? If Stavanger University Hospital is a level 3 hospital, there should be other delivery and neonatal wards in the region that make referrals (i.e. extreme preterm births) to Stavanger.

Reply: We appreciate this comment and fully agree with the reviewer that “level 3” might be misleading. The use of the term “level 3” in this case was to reflect that Stavanger University Hospital also provides care for high risk deliveries (i.e. extreme preterm births), except for prenatal identified severe malformations needing surgery immediate after birth. We have removed the term in the revised manuscript.Stavanger University Hospital is the only delivery unit in southern Rogaland County, and occasionally admits women in risk of extreme preterm birth (

Page 4 Line 26: Fisher&Paykel not Fiscer&Paykel

Reply: We have corrected this in the manuscript.

Page 5 last paragraph: “During video analysis, the newborn`s respiratory effort at placement at the resuscitation bay was characterized as adequate, inadequate (e.g. grunting or severe retractions) or apnoeic. Drying and stimulation was considered adequate if the newborn received tactile truncal stimulation (drying, chest- and back rubs) prior to respiratory support”

Comment: Did PAB use a standardized score sheet? Were some of the recordings analyzed by (at least) two independent researcher to ensure reliability?

Reply: As replied in the second comment of this reviewer, we issued a SOP and a standardised registration sheet for this study. With exception of the evaluation of the newborns respiratory effort and whether initial stimulation was adequate, all data addressing the aims of this study was quantitative measures. Therefore, we found the use of a single reviewer appropriate.

Analysis (page 6): I suggest that the authors include what statistical tests they used. This information can be found in the figure legends, but should perhaps also be mentioned in the methods section.

Reply: We have changed the manuscript in accordance with the reviewers’ suggestions.

Page 6 line 35: its’ legal and ethical concerns have been debated

Comment: Should be ‘its legal and ethical concerns have been debated’

Reply: We have corrected this in the manuscript.

RESULTS

Page 7 Line 48: Respiratory support alone, either by CPAP or PPV was sufficient in 95% of the resuscitated newborns, whereas the remaining 5% received (additional) circulatory support by chest compressions (n=10) or intravenous fluid and/or adrenaline boluses (n=15).Comment: I suggest deleting the word “additional”

Reply: We have changed the manuscript in accordance with the reviewers’ suggestions.

Page 8 Line 30: “Upon arrival on the resuscitation bay, 161/192 (82%) newborns were adequately dried”

Comment: Why is the denominator 192, and not 196? Typo?

Reply: We apologize for what is indeed a typo. We have corrected this in the manuscript.

Page 8 Line 50: “Median intubation time for each attempt was (of) 40 seconds (33-55)”

Comment: I suggest deleting the word “of”

Reply: We have changed the manuscript in accordance with the reviewers’ suggestions.

Page 9 Line 3: “Eight of the newborns who received full cardiopulmonary resuscitation were video-recorded”

Comment: How many newborns received full CPR in total? Please also define “full cardiopulmonary resuscitation”.

Reply: With “Full cardiopulmonary resuscitation” we intended to address the newborns receiving chest compressions with or without adrenaline. In total, 10 newborns received chest compressions during our study period. We agree in the confusion of mixed notions. We have defined “full cardiopulmonary resuscitation” and have replaced it with “chest compressions” in some places in the revised manuscript.

Page 9 Line 43: “Pneumothorax was diagnosed in three of the resuscitated newborns.”

Comment: Did pneumothorax occur in infants that received PPV in the delivery room? Complication to resuscitative interventions?

Reply: This is an interesting comment. All three newborns with diagnosed pneumothorax in the group from 34 weeks of gestation, received PPV. There were additionally three newborns in the group <34 weeks of gestation (32, 28, and 27) that were diagnosed with pneumothorax, of which two were treated with CPAP only. Pneumothorax is certainly a known complication of both PPV and CPAP during resuscitation, but could also be a result of RDS and subsequent CPAP treatment during NICU stay. Although it is a very interesting question, our study was not designed to draw any conclusion.

DISCUSSION

Page 10 Line 52: “In our study, more than 30% of the newborns receiving CPAP were evaluated as adequate breathers by the viewer, highlighting the uncertainty of the necessity.”

Comment: How was “adequate breathers” defined? Maybe they needed supplemental oxygen or were having mild retractions that could not be seen in the video recordings?

I would suggest revising the term “uncertainty of the necessity”

Reply: This is an important comment. The breathing effort at arrival at the resuscitation crib was defined as apneic, inadequate (i.e. grunting or severe retractions) and adequate breathers. The latter group includes newborns with mild to moderate retractions as well as newborns crying at placement at the resuscitation crib. The indication for CPAP during newborn resuscitation is not clearly defined, and the decision to provide CPAP could depend on more than we could observe in our video recordings. As CPAP were provided in newborns with various respiratory effort at arrival on the resuscitation crib, we have throughout the manuscript deliberately used the term “provided” and not “needed”. We have rephrased the paragraph in the revised manuscript.

Page 11 Line 12: ‘different patient`s characteristics, is unknown’

Comment: Should be ‘different patient characteristics, is unknown’. Niles et.al. should be Niles et al.

Reply: We have corrected this in the manuscript.

Page 11 Line 24: “Our findings are similar to previous findings from Norway by Skaare et.al. for both incidence and duration of PPV (14, 23)”

Comment: I think perhaps the authors could be more clear about the rationale for performing a very similar study in a similar context as the one reported by Skaare et al

Reply: As these studies are based on real-life observations of interventions with a relatively low incidence, it is appropriate to report from comparable settings to verify the findings and hopefully strengthen the validity of the results. While our studies are both conducted in settings with similar resources and training, there are however some differences: Skaare et al reported newborn resuscitation in 1507 newborns from three hospitals in eastern Norway in three subsequent time periods. All three hospitals differ in the level of prematurity they provide care for. A registration form was filled out only for newborns transported to the resuscitation crib for support after birth. Our study was conducted at the only hospital in our region, in an uselected population, providing care from GA 23. Importantly, to ensure complete registrations and prevent “missed cases”, we registered an incident report form for every newborn, i.e. also were no interventions were provided which strengthens our study. These issues could have been elaborated in the introduction, but due to the length of the manuscript and word limitations we find it difficult, and these issues has no impact on the results of our study.

Page 11 Line 40: ’the recommended time of PPV before initiating chest compressions were prolonged’

Comment: Should be ‘the recommended time of PPV before initiating chest compressions was prolonged’

Reply: We have corrected this in the manuscript.

CONCLUSION (not CONCLUTION)

Reply: We have corrected this in the manuscript.

Page 13 Line 6: “The high incidence of resuscitative interventions in premature newborns ≤34 gestational weeks supports centralizing those deliveries to centres with adequate expertise and high level of preparedness to provide optimal stabilization and resuscitation at birth.”

Comment: Do the authors suggest that all deliveries ≤34 gestational weeks should be centralized? What is ‘adequate expertise’?

Reply: As discussed in the initial comments from this reviewer, we agree that our data might be insufficient to support such a statement, and have removed it in the manuscript.

Table 4: Are the base excess values supposed to be negative (base deficit)?

Reply: We apologize for this misspelling. The reported values were Base Deficit and not Base Excess. We have corrected this in the manuscript.

In conclusion, a well-written paper, and probably a well conducted study. However, more information about the data analysis should be provided to judge the methodological quality. I think that the study conclusions could be improved. Based on the rich data obtained with the incidence report forms and video recording, I expect the authors to make suggestions for practice that are even more interesting and relevant. E.g., I find the higher need for intervention in post term infants interesting. Do the authors think that this finding should inform obstetrical practices (when to induce post term pregnancies)? What do other studies tell us about post term pregnancies and neonatal compromise?

Reply: We would like to thank you for a thorough review. There is to our knowledge no other studies presenting the incidence of resuscitative interventions stratified for different gestational age groups. We hope this manuscript inspires further studies to clarify on the important issues you address, e.g. regarding increased incidence during post-term pregnancies.

Reviewer: 3

Many thanks for your hard-work in defining the incidence of newborn resuscitation in your regional population. I have made suggestions for both major and minor revisions to your manuscript, which I found informative.

General comments which support consideration of this study's publication after revision:

Incidence of newborn resuscitation in high, middle and low resource settings has been previously reported. The major potential advantage of this study over other published works is the combination of prospective data collection with video analysis. Whilst the study is based within a small regional population, it has a relatively large series of video analysed resuscitations which enhances the validity of reported incidences of delivery room interventions and their characterisation. Other similarly large published series of video analysed resuscitations have reported only on one aspect of resuscitation (such as infant position or suctioning).

Overall, this paper has the potential to overcome some of the limitations which are present in our current evidence base:

- Studies where data collection of resuscitation events is based on retrospective review of medical notes (subject to underreporting & inaccurate notation)

- Studies based on solely high-risk sub-populations; reducing generalisability

- Video-analysis studies which have included only small study samples, have focused on healthcare professional feedback & learning, are limited to a particular sub-population, or focus on only one or two interventions

- Studies in which outcomes have not been declared

In its present form, however, the manuscript has not fully utilised the opportunities presented by video-recording delivery-room resuscitation events. Whilst incidence is reported, it does not fully characterise resuscitation interventions.

The following major and minor revisions are suggested to improve the manuscript further.

Suggested improvements for Authors:

Major Revisions:

1) The first objective of this study is I) to describe the incidence and characteristics of newborn resuscitative interventions during the first minutes of life.

Incidence of CPAP, PPV, Chest compression, IV adrenaline and IV fluid bolus are described. However, further characterisation of the resuscitation events is needed within the results section.

Depending on the availability of concurrent physiological parameters, and definition of video recordings, the following should be considered for inclusion in the study report where possible:

Timing of intervention from birth, response of infant & quality of delivered intervention:

Primary resuscitative interventions:

-Time from birth to arrival at resuscitation crib -Time from birth to initiation of PPV -Time from when PPV started to when chest rise first seen or heart rate increase seen?

-Time from birth to initiation of chest compressions -In infants who received chest compressions – how long had effective PPV been administered prior to starting chest compression?

-Time from birth to first attempt at IV/IO access -Time from birth to first successful IV/IO access (and what type?) -Time from birth to first adrenaline dose / Fluid bolus -In apnoeic infants with bradycardia or without cardiac output; time to first detectable heart rate, time to HR>100 bpm, time to gasping, time to spontaneous breathing?

Secondary resuscitative interventions (of lesser importance but will still be of interest to many readers):

-Time from birth pulse-oximetry attached & time that reliable pulse-ox signal established -Time from birth ECG attached (if attached) -Frequency/time interval of Heart rate checks by stethoscope if no reliable ECG/Pulse-oximeter -Time from birth CPAP started (in CPAP only group) -Duration of CPAP delivered in CPAP only group -Initial grade of breathing (inadequate/adequate) + response to CPAP in CPAP only group (did they become adequate breathers?) -How many infants were started on CPAP without PPV but then subsequently became apnoeic?

-Incidence of airway maneourves needed to achieve chest rise: Jaw thrust/oropharyngeal airway/ two-person facemask hold & PPV technique -Starting FiO2 (air or more?) -Incidence of suction: use of laryngoscope during suction?

-Incidence of response to suction (Distress in breathing improved? PPV subsequently successful or bradycardia occurred?) -Quality of chest compressions delivered (Rate, depth, recoil) -Ratio of compressions to breaths used -Dose of first and subsequent adrenaline boluses -Number of adrenaline boluses

The above data could be presented almost entirely in tabular form or graphically as a box and whisker plot.

Reply: We appreciate this comment which is highly understandable. As the reviewer rightfully points out, the study resulted in a wider amount of data than presented in this paper. We considered including all results in one manuscript, but result were too numerous for that and would limit the focus on the present results. We have therefore split the results into two publications with different objectives: This one covering the incidence, characteristics and short term outcome, and a second article addressing the timing of interventions, patient monitoring during newborn resuscitation, and adherence to resuscitation guidelines.

Currently there are no published prospectively collected/video scrutinised data on timing and quality of full resuscitation interventions in the delivery room. By providing more in-depth characterisation of resuscitation interventions, the authors' would fulfil the potential offered by video-recording the resuscitations, and fulfil their primary objective. In addition, the authors' discussion points would be strengthened, for example:

-Page 10, line 52-56: "In our study, more than 30% of the newborns receiving CPAP were evaluated as adequate breathers by the viewer, highlighting the uncertainty of the necessity." – These data are not presented in the results section, only here in the discussion for the first time. In addition, no distinction is made between CPAP use in term and preterm infants; The Norwegian Resuscitation guideline advises that Initial respiratory support of spontaneously breathing preterm infants with respiratory distress may be provided by CPAP.

Reply: This is an important comment. As commented in the reply of the 2nd reviewer, CPAP was provided in newborns with various respiratory effort at arrival at the resuscitation crib. In accordance with the reviewers' comment, we have clarified the manuscript by adding a table presenting the provided respiratory support by the newborns respiratory effort at birth.

-Page 11, Line 35-39: "Still, three of the newborns in our study received chest compressions for less than one minute, suggesting proper airway handling might have been sufficient." - Can this be concluded if there is no comment on how the airway and ventilation of the infants were managed? In these 3 infants with <1 min chest compressions, how long had effective PPV been administered prior to initiating chest compressions? This data should be provided in the results section.

Reply: We appreciate this comment and have rephrased in the manuscript.

-Page 11, Line 39-47: "When ILCOR revised their guidelines on newborn resuscitation in 2015, the recommended time of PPV before initiating chest compressions were prolonged, recognizing that most compromised newborns will respond to adequate ventilatory support alone (4). Our findings may support this recommendation." - The findings of this study can only lend support to this recommendation if data are provided on duration of effective PPV prior to initiation of chest compressions, and the effect on heart rate. I note the 2015 revision of the NRR recommends: 'Start chest compressions only after you are sure that you have ventilated the child adequately for 90 - 120 seconds'. Were there infants in your population who were bradycardic up until 90-120 seconds of PPV but then were no longer bradycardic and thus did not require chest compressions?

Reply: We fully agree in this comment. We unfortunately do not have registrations on the newborns heart rate the first minutes of the resuscitation, but have added the time interval from onset of PPV to chest compressions in the table.

-Page 8, lines 52-54: "failed mask ventilation n=4, chest compressions (n=3) or need for prolonged respiratory support (n=3)" - Any indication of why face mask ventilation had failed? Technique, anatomy? How long is prolonged respiratory support?

Reply: Failed mask ventilation occurred when the provider of ventilation did not acquire sufficient aeriation of the lungs, verbally stated on the video recording by the provider her-/himself. We did not further speculate in the reason for failed mask ventilation which could have had different explanations (i.e. obstructed airways, stiff lungs, inappropriate technique). Intubation due to (expected) prolonged respiratory support was registered when a newborn, adequately ventilated by mask, was intubated to secure the airways prior to transportation to NICU. We did not define a certain time limit.

2) Short-term outcomes should be defined within the manuscript's method section.

The second objective of this study is II) to assess short-term outcomes after resuscitation.

Short-term outcomes are not defined within the manuscript's methods, but are reported as 'immediately returned to parents' Vs 'NICU admission' for infants ≥34 weeks gestation in the results section, alongside NICU length of stay, need for mechanical ventilation, occurrence of pneumothorax, provision of therapeutic hypothermia, Magnetic Resonance brain scan reports & death.

Within the discussion, short-term outcomes are described as ‘Survival with rapid recovery, need for intensive treatment or death’.

Short-term outcomes are not differentiated for infants <34 weeks’ gestational age.

Consistent definitions of short-term outcomes should be used and declared for all gestations. (e.g. Survival – returned to parents, survival – Neonatal Unit admission (& characterise level of intensive care), Length of stay, and Death in first 28 days)

Reply: We appreciate this comment and have added a consistent definition of short-term outcomes in the revised manuscript. It is, however, our opinion that newborns <34 weeks of gestation should not be included in the results describing the short term outcomes and complications during NICU stay for three main reasons: 1) they are routinely admitted to our NICU after birth, regardless of interventions, 2) resuscitative interventions in premature newborns could be considered as stabilization measures due to prematurity, and 3) short term outcomes and level of intensive care upon admission to NICU are strongly related to their prematurity and are difficult to differ from consequence of resuscitative interventions from our results. We appreciate this comment however, and have clarified this in the manuscript.

3) A stronger acknowledgement of the study’s limitations is required within the discussion.

In particular:

-Video-analysis was achieved in 196 of a total of 295 resuscitations: 33.6% loss of video-analysis overall.

-20 intubations took place in the study period, only 10 of these were video-analysed: 50% loss to video-analysis.

-10 infants received full cardiopulmonary resuscitation, 8 of these were video-analysed: 20% loss to video-analysis

-Page 12, line 38-40. "Video cameras were not available at all resuscitation bays, but this should not cause a bias. There was a poor response rate when consent was asked by letters. This typically included newborns who were discharged early from hospital, and may represent a group where lesser interventions were required. Importantly, the results regarding incidences of resuscitative interventions were not consent- or video-dependent and therefore not affected by these potential biases" – This requires further qualification. If loss of video analysis is not a cause for potential bias or inaccuracy, why use video-recording at all? Loss of video analysis occurred in 50% of infants who were intubated and 20% of infants who received full resuscitation – these are infants who do not ‘represent a group where lesser interventions were required’. In infants who had video-analysis, did the recorded interventions in the ‘observational incidence report form’ correlate with the video analysis? (This would provide a measure of risk of bias in the 33% of resuscitations within this population for which video analysis was missed)

Reply: The reviewer appropriately address the limitation and potential biases of missed video recordings. As the incidences of different resuscitation measures reported in the manuscript is solemnly based on the incidence report form, the missing videos does not affect these results. The incidence report form was not consent dependent, and was filled out for every birth – also when no resuscitative intervention was provided. We crosschecked the reports against the hospital birth records to ensure that all newborns were included. Where video records were eligible, the report forms were consistent with the recordings. The incidence report form will be included in the revised manuscript as suggested. All further specifics (e.g. duration of interventions, number of adrenaline doses, intubation attempts, etc.) was recorded during video analysis, and could have been biased by missing video records.

The statement that missed cases “represent a group where lesser interventions were required” was aiming at the newborns where consent was not obtained during hospital stay, and mainly represented newborns discharged from the hospital within a few days (consent was obtained two-three times weekly). It was our consideration, that this did not include severe compromised newborns with subsequent need for intensive care treatment. As the concern of missed video recordings is also rightfully addressed by other reviewers, we have elaborated on this in both the method- and the limitation section of the manuscript.

4) The discussion should further exploration how generalisable the findings of this study are to other populations.

The authors may like to comment/compare how the incidence of fetal distress, as characterised by umbilical cord gases & 1-minute Apgars, in this population compares with international data. From the data presented in the manuscript, evidence of fetal distress appears low – even in the eight infants who received full cardiopulmonary resuscitation, only one infant demonstrated severe cord blood acidaemia (pH<7.00), and three (38%) had arterial cord pH ≥7.2.

The caesarean section rate was 13.7%, with 9.6% acute caesarean sections – the authors should reflect in their discussion on how their population compares with other high resource settings and if they feel this may affect the generalisability of their findings.

Reply: This is an important comment. We are not aware of other studies evaluating umbilical cord gases in resuscitated newborns. As the reviewer rightfully points out, both the Apgar and umbilical cord gases suggest that most resuscitated newborns were not in severe fetal distress and have had an adequate placental gas exchange. This imply that many of our resuscitated newborns could have destabilized at the time of birth or shortly thereafter, in need of brief airway support to initiate spontaneous breathing (primary apnea), or even represent newborns who would have profited on drying and stimulation alone without the need of resuscitative interventions. Both cases are plausible and did probably occur. Our Caesarean section rate is low compared to other high-resource settings, for example, Niles et al reported a 31% Caesarean section rate in their cohort. Still, the newborns delivered through Caesarean sections accounts for approximately 40% of all resuscitated newborns in our study. However, this is mainly acute Caesarean sections. Planned Caesarean sections accounted for 4.1% of the total births and 5% of the resuscitated newborns. With exception of the study from Skaare et al., which was comparable to our findings, we could not find data on the fraction of acute Caesarean sections in similar studies. We appreciate the suggestions of the reviewer, and have elaborated on this in the discussion section.

Minor Revisions:

Grammar/spelling/writing style:

Page 3, Line 55 “Stavanger University Hospital serves a population of 350.000” – suggest 350,000

Reply: We have corrected this in the manuscript.

Page 3, Line 35-37 “Further, short-term outcomes of near- or at term newborns after resuscitation are poorly described (17, 18)” – Consider revising the word ‘Poorly’ - use of the adjective ‘poorly’ implies criticism of prior published works - is this the intention or is the intention to highlight a gap in the current literature?

Reply: The intention was to highlight a gap in the literature, and we have changed the manuscript in accordance with the reviewers’ suggestions.

Page 4, Line 6 "Newborns with antenatal diagnosed severe" – suggested ‘antenatally diagnosed’

Reply: We have changed the manuscript in accordance with the reviewers’ suggestions.

Page 4, Line 19 "and a backup resuscitation bay in case of for example twin deliveries" – suggest "and a backup resuscitation bay in case of, for example, twin deliveries"

Reply: We have changed the manuscript in accordance with the reviewers’ suggestions.

Page 6, Line 18 "Results are presented as number (%), median (IQR) and mean (95% CI)." – suggest defining the abbreviations IQR & CI at first use.

Reply: We have changed the manuscript in accordance with the reviewers’ suggestions.

Page 7, Line 43: table description "*Umbilical blood sampels were not available for all patients" – samples

Reply: We have corrected this in the manuscript.

Page 10, line 24 - table description, Fisher's exact test not fischers.

Reply: We have corrected this in the manuscript.

Page 10, Line 47-49 "We defined CPAP as a resuscitation intervention, as it is suggested in the ILCOR guidelines as a mean to” – suggest ‘ as a means to’

Reply: We have corrected this in the manuscript.

Page 10, Line 55-57 “highlighting the uncertainty of the necessity. ” – suggest ‘highlighting the uncertain necessity of the intervention’

Reply: As discussed in the comments from reviewer 2, the sentence has been rephrased in the manuscript.

Page 12, Line 52 "CONCLUTION" – Conclusion

Reply: We have corrected this in the manuscript.

Page 13, Line 26 "AUTOR CONTRIBUTION" – Author Contribution

Reply: We have corrected this in the manuscript

Methodological descriptions:

Please specify if any high-risk deliveries were transferred in from other regions.

Reply: As discussed in the comments from reviewer 2, we have elaborated on this in the method and result section.

The authors should define how CPAP was delivered – was it humidified, nasal or face mask or simply by applying the NeoPuff T-piece resuscitator with adjustable PEEP valve?

Reply: CPAP was indeed delivered with the T-piece resuscitator with adjustable PEEP valve. This has been clarified in the manuscript.

“Full cardiopulmonary resuscitation” is used on several occasions without full definition; please define on first use or within the methods section.

Reply: As discussed in the comments from reviewer 2, we have clarified this in the improved manuscript.

Page 6, Line 42 "For patient and personnel safety considerations" - what safety considerations mean video recordings need to be erased after 3 weeks? Should this be for data-regulation /privacy purposes?

Reply: We appreciate the suggested clarification. This was meant to address the demand from the hospital data protection officer to ensure patient and personal privacy. This has been rephrased in the manuscript.

“Resuscitation Bay”: Bay usually defines a physical space or alcove. A “Resuscitation bay” would normally describe the physical space in which the equipment is stored/used, as opposed to the equipment itself. Resuscitation cot/crib/unit may be more appropriate terms?

Reply: We appreciate the suggested term and have changed this throughout the manuscript.

Scientific Corrections/clarifications:

Page 7, Line 3: "During the study period, 4610 deliveries took place in our region, resulting in 4697 live born" – Should this be ‘4710 deliveries took place’?

Reply: The 4697 newborns also includes 85 twin deliveries and one set of triplets, resulting in a higher number of newborns compared to deliveries.

Page 7, Lines 4-5 "12 were transport deliveries" – what are transport deliveries?

Reply: Transport deliveries referrers to deliveries taking place during transport to the hospital (i.e. in an ambulance). We apologize for the imprecise translation, and has rephrased the sentence in the manuscript.

Page 7, Lines 17-19, "In total, 295 (6.3%) newborns received resuscitative interventions. The characteristics of the resuscitated newborns are shown in Table 1." Readers may be confused that only 256 participants are included in table 1, when earlier in the sentence 295 are referenced. Consider clarifying that table 1 includes only the infants for which consent was provided to access further details?

Reply: We appreciate the suggested clarification and have rephrased the heading in the table.

Base Excess: In the manuscript, many of the reported Base Excesses are positives, even in infants with acidotic pH. Should these Base Excesses be negative? Presumably the acidotic infants did not have an excess of base (i.e. an alkalosis) and so had a negative base excess?

Reply: We apologize for this misspelling. The reported values were Base Deficit and not Base Excess. We have corrected this in the manuscript.

Amendments to tables:

Table 1

Consider presenting the normal range (2 Standard deviations) of umbilical cord gas data (or alternatively the IQR and minimum and maximum values) as the IQR doesn’t provide information on the 50% of the infants who had the most extreme or most normal gases.

Reply: We appreciate this suggestion and have now presented the umbilical cord gas data as Mean (SD).

Table 2

PPV on 28-33 gestation group: n=26.Percentage also stated as 26% - should be 40.6%?

Reply: We apologize for this type-error. We have corrected this in the manuscript.

Table 3

Why have a "-" to indicate no adrenaline bolus for four infants and a "0" for one infant?. Consider including Apgar scores.

Reply: The "-" was ment to indicate that the newborns did not recive any i.v./i.o. access, hence adrenaline boluses was not an option. The one infant with "0" had i.v. access but did not receive any adrenaline. We have improved the table by including both i.v./i.o. access and Apgar scores.

Table 4

Page 10 Table 4: "Table 4 - Comparison of newborns above 34 weeks gestation returned to parental care or admitted to newborn intensive care unit (NICU)" - Should be equal or above 34 weeks / newborn or neonatal (as described on page 4)?

Reply: We apologize for this misspelling. We have corrected this in the manuscript.

Statistical analysis: Why was Chi-Square used for some data points and Fisher's Exact used in others?

Reply: Fisher`s Exact test was used when comparing categorical variables where expected count was < 5. This has been clarified in the method section.

References:

Page 6, Line 35: Reference 22 does not appear related to the ethics of video recording

Page 12, Lines 13-24: Reference 29 is not primarily related to functional residual capacity or primary apnoea after birth

Reply: We highly appreciate this observation. There are two references made in the manuscript not relating to the subject. We apologize for this, and have replaced them with the relevant papers originally meant for the references. This must have happened during the writing process and should have been discovered prior to submission. All references are now thoroughly checked.

Title:

Consider defining the population within the study title (readers may anticipate a higher participant number from the current title) – For example: 'Incidence of newborn resuscitative interventions at birth and short-term outcomes – A regional population-based study'

Reply: We agree, and the title has been changed according to the reviewers suggestion.

Consider as electronic supplementary files:

Photo of the resuscitation bay & equipment

Copy of Norwegian Resuscitation Council Guideline

Copy of observational incidence report form

Reply: We appreciate these suggestions and have added the Norwegian Resusciation council Guideline as reference and the other files as supplementary material.

Funding:

“PAB received a PhD grant from Laerdal Foundations” - Please clarify whether Laerdal had any involvement in study design, analysis or conclusions?

Reply: We would like to thank you for a thorough review. Laerdal Foundation had no involvement in the design, conduction, analysis or conclusions in the study. This has been clarified in the manuscript.

Reviewer: 4

I confine my remarks to statistical aspects of this paper.

These were very simple, but appropriately so. I recommend publication.